consisting of two layers corresponding with those usually termed decidua vera and reflexa, was found in the tube, adhering to its inner surface and surrounding the placenta and villi of the chorion. The following is the description given by the author of the appearances observed in the last of these cases:—"The uterus was enlarged, and the whole lining membrane coated with a thick irregular layer of a substance resembling the fibrine of the blood, of a red colour, in the upper part. The right Fallopian tube about the middle was as large as a walnut, or larger where its coats had burst, and a coagulum of blood was hanging through the irregular aperture. The tube was pervious from the corpus fimbriatum to the dilated part. On cutting open this expanded portion, a small embryo enclosed in the amnion was observed, and the vesicula umbilicalis, remarkably large, with its peduncle, came into view. All the cells of the placenta and villi of the chorion were seen distended with coagulated blood and surrounded with a deciduous membrane, a great part of which has been separated from the inner surface of the tube."

## XVI. "Experimental Researches on the Conductive Powers of various Substances, with the application of the Results to the Problem of Terrestrial Temperature." By William Hopkins, Esq., M.A., F.R.S., of St. Peter's College, Cambridge. Received June 10, 1857.

(Abstract.)

1. The author remarks, that in giving an account of these experimental researches, it is first necessary to define strictly the manner in which the *conductivity* or *conducting power* of a substance with reference to heat, is accurately *measured*. For this purpose, conceive the conducting substance to be bounded by two parallel plane surfaces of indefinite extent, the distance between them being $h$. Suppose one of these bounding surfaces (which, for convenience, may be called the *lower* one) to be kept at a uniform and constant temperature $t_1$; let the temperature of the *upper* surface be also constant and uniform, and equal to $t_2$; and let $\tau$ denote the temperature of the free space into which the heat radiates from the *upper*

surface. Then, if we denote the *conducting power* of the substance by $k$, and the *radiating power* of its upper surface by $p$, we obtain by mathematical investigation,

$$\frac{k}{p}=\frac{t_2-\tau}{t_1-t_2}h.$$

It is here supposed that $k$ is independent of the temperature of the substance, and that $p$ is equally independent of that of the surface from which the radiation takes place. It may also be remarked, that the quantity of heat which radiates from a unit of area in a unit of time, is measured by the product of $p$, and the difference of the temperatures, $t_2$ and $\tau$, of the radiating surface and surrounding medium. It is the ratio $\left(\frac{k}{p}\right)$ which the conducting bears to the radiating power, which has more frequently been determined in researches of this kind; but this would not have sufficed for the author's object, which has been the determination of the values of $k$ for different substances. The radiating power ($p$) probably varies for different substances as much as the conductive power ($k$), but all consideration of the former power will be avoided if we suppose the radiating surface of the substance to be covered with a thin layer of some given substance which shall take the temperature of the upper surface of the substance itself, and from which the radiation shall always take place, whatever be the nature of the substance experimented on. Thus if $c$ denote the radiating power of the superimposed thin layer (which was mercury in these experiments), we shall have

$$\frac{k}{c}=\frac{t_2-\tau}{t_1-t_2}h;$$

a formula which ($c$ being always the same) enables us to compare the conducting powers for different substances, or to determine their absolute numerical values when that of $c$ is once determined. In the actual experiments some error was necessarily superinduced by the necessity of working with portions of the different substances of comparatively small instead of indefinitely large horizontal extent, such as strict mathematical accuracy would require. This error, however, was undoubtedly small, and, moreover, can have had extremely little effect on the *relative* values of $k$, since it must have

been nearly the same for all the substances on which the experiments were made.

The apparatus made use of was sufficiently simple. The heat was derived from a stove, the fire within which could be elevated, depressed, or entirely withdrawn at pleasure. A very shallow pan of mercury was placed over the stove, the fire being so regulated as to preserve the mercury at any constant required temperature. A cylindrical block of any substance, the conductive power of which was to be determined, was so placed as to rest with its base just in contact with this mercury, from which it derived its temperature ($t_1$). Its upper end was also covered with sufficient mercury just to cover the small bulb of a thermometer. The temperature of this latter mercury gave $t_2$. Careful arrangements were made for observing these temperatures, as well as that of the air into which the heat radiated from the upper mercury. Precautions were also taken to prevent the lateral transference of heat through the sides of the block, and any influence of radiation from the heated stove which might affect the results of the experiments. When the temperature ($t_2$) of the upper mercury became stationary, the experiment was completed, and the substitution of this stationary value of $t_2$, together with the values of $t_1$ and $\tau$ in the above formula, gave the numerical results required.

2. The following were some of the results obtained for conductive powers as measured by the ratio $\frac{k}{c}$:—

| | |
|---|---|
| Chalk............ | ·056 |
| Clay ............ | ·07 |
| Sand ............ | ·15 |
| Sand and clay .... | ·11. |

These substances were all in the state of *very dry powder*. In the last case the sand and clay were in equal quantities.

*Substances in the state of rock-masses.*

(1) *Calcareous rocks.*

| | |
|---|---|
| Chalk (same block from a dry state to a state of saturation with water) from .......... | ·17 to ·30 |
| Oolites from Ancaster (dry to saturated) .... | ·30 to ·40 |
| Hard compact limestones ................ | ·50 to ·55 |

| | |
|---|---|
| (2) *Argillaceous substances.* | |
| Clay, very dry to very moist | ·23 to ·37 |
| (3) *Siliceous rocks.* | |
| New red sandstone (same block dry to saturated) | ·25 to ·60 |
| Freestone | ·33 to ·45 |
| Hard compact sandstones (Millstone-grit) | ·51 to ·76 |
| (4) Hard, compact, old sedimentary rocks | ·50 to ·61 |
| (5) Igneous rocks | ·53 to 1·00 |

*Effect of Pressure.*

3. This effect was not appreciable for a pressure of 7500 lbs. per square inch in such substances as bees'-wax and spermaceti. Nor was there any sensible effect with chalk between a pressure of 4300 lbs. and 7500 lbs. per square inch.

Clay which when incompressed had a conducting power $=\cdot26$, had when compressed with 7500 lbs. per inch, a power $=\cdot33$; and the conducting power of a mixture of sand and clay in equal quantities rose from ·36 to ·378 by an increase of pressure from 4300 lbs. to 7500 lbs. per inch.

Generally the effect of pressure is much less than might have been anticipated.

*Effect of Discontinuity.*

4. When the conducting mass consists of a number of strata superimposed on each other, the mathematical problem presented to us requires a distinct investigation, which is here given under a very general form, together with the experiments necessary to determine the effect of this kind of discontinuity. The result is that if a mass of sandstone consisted of a number of strata, the conducting powers of which should be about ·5, the mean conductivity of the whole would not be diminished by more than about $\frac{1}{20}$th part, supposing the average thickness of strata to be 1 foot; or by about $\frac{1}{10}$th, if that average thickness should be 6 inches. This effect is much less than might possibly have been anticipated.

*Effect of Moisture.*

5. This effect was very considerable in those rocks which are great absorbents of water. The maximum effect appears to be produced

by a quantity of moisture which falls considerably short of producing complete saturation. The conducting power of a piece of dried chalk was =·19, but became =·30 when the substance was very moist. That of a well-dried piece of new red sandstone was =·25, but became as much as ·60 when saturated. Both these substances absorbed a large quantity of water. Ancaster oolites absorbed considerably less, and their conductivity was affected in a smaller degree. For a block of dry clay the conductive power was ·23, and became ·37 when well moistened. Close indurated sandstone, palæozoic rocks of close texture, and igneous rocks are bad absorbents, and are very little affected in their conductive powers by moisture.

*Comparison of Deductions from Theories of Terrestrial Temperature with the Results of Observation.*

6. It has long been established by mathematical investigation, that if a large globe like the Earth be heated in any manner and in any degree, its temperature at points not too remote from its surface, and after a sufficient lapse of time, will necessarily become such that the increase of temperature in descending along a vertical line will be proportional to the increase of depth. In this enunciation, however, it is assumed that the conductive power throughout the mass, or at least throughout its more external portion, is uniform. The difference of conductive power between the unstratified and sedimentary portion of the earth's crust, or that between one sedimentary portion and another, has not hitherto been taken into account*. The author has investigated the problem assuming the crust of the globe to consist of any number of strata of different conductive powers and bounded by parallel surfaces, the problem being much simplified by considering their surfaces as plane instead of spherical. Then, assuming the temperature of the crust of the globe to be due entirely to the transference of heat from its central portions to its surface, it is shown that the increase of temperature in descending vertically through any two strata, ought to be in the inverse ratio of the conductive powers of those strata, whether the two strata belong to the same group of stratified beds, or to two different groups in different localities. Such at least must be the result unless we introduce very

* Except in the case in which Poisson investigates the state of temperature of a sphere surrounded by a single concentric spherical shell of different conductivity.

arbitrary and, as the author conceives, entirely inadmissible hypotheses into the problem.

For the purpose of testing this theory in its application to our own globe, four or five cases of Artesian wells and vertical shafts are especially referred to, in which the temperature has been carefully observed at greater depths than at any other places in Western Europe, or probably in any other part of the globe*. The cases spoken of are the following:—

(1) An Artesian well near Geneva.—Depth=225 metres; increase of depth for 1° (F.)=55 feet.

(2) An Artesian well at Mondorff in the Grand Duchy of Luxembourg.—Depth=730 metres; increase of depth for 1° (F.)=57 feet.

(3) An Artesian well at New-Saltzwerk in Westphalia.—Depth=644·5 metres; increase of depth for 1° (F.)=54 feet.

(4) The Puis de Grenelle at Paris.—Depth=546 metres; increase of depth for 1° (F.)=60 feet.

(5) A coal shaft at Duckenfield, near Manchester.—Depth=1400 feet; increase of depth for 1° (F.)=65 feet.

(6) A coal shaft at Monkwearmouth.—Depth about 1700 or 1800 feet; increase of depth for 1° (F.) about=60 feet.

The general rate of increase of temperature in our own deeper coal-mines is that of about 1° (F.) for 60 feet in depth; and the same result has been obtained for many parts of the chalk in Northern France.

These cases present a remarkable approximation to uniformity, whereas the conductive powers of the strata which have been penetrated are very different. Cases (4) and (5) present the best means of comparison. The Puis de Grenelle passes through nearly 500 metres of chalk, the conducting power of which is estimated by the author at somewhat more than ·25, while the mean conducting power of the rocks through which the coal shaft at Duckenfield passes, is estimated, by means of experiments performed on specimens of these rocks, at rather more than ·5. This is about twice as much as in the former case, whereas the depths corresponding to the same increase of temperature are only as 65 to 60, instead of being in the ratio of about 65 to 35, as they ought to be according to the

* In a great majority of instances observations of this kind have not been made with sufficient care to be relied on.

theory here considered. In all the other cases the conductive powers of the masses penetrated are doubtless greater than that of the chalk at Paris, though, for the most part, they present a *more* rapid increase of temperature in descending, instead of a *less* rapid increase (as this theory would prescribe) than the Puis de Grenelle.

Within the region comprising the cases above cited, there are many local variations as to the rate of increase of terrestrial temperature in descending below the earth's surface. The author conceives that these phenomena cannot be accounted for according to this theory without the introduction of arbitrary hypotheses.

Upon the whole, he believes that in the present state of our knowledge of terrestrial temperature, it is impossible to account for its phenomena by regarding them as the consequence simply of heat, not generated in, but transmitted through the crust of the globe from some deep-seated central source.

The discrepancy between the actual terrestrial temperatures and those which would be assigned by the theory here discussed, may be illustrated perhaps by placing the subject in a rather different point of view. It is assumed in the theoretical investigation, that the isothermal surfaces at depths sufficiently great (as 50 or 100 miles for example) are approximately concentric with the earth's external surface, or, speaking with reference to areas not too large, parallel to that surface, in which case it is proved that the isothermal surfaces at comparatively small depths (not much exceeding that of the sedimentary beds) cannot be parallel to the external surface. For example, the depth of an isothermal surface of given temperature, which should be some 3000 feet at the Puis de Grenelle, ought to be nearly 6000 feet at the coal shaft at Duckenfield; and at other places it ought to be very nearly proportional to the conductive power of the terrestrial mass lying above it. But the observations above cited demonstrate that, independently of local irregularities, such an isothermal surface is nearly at equal depths throughout the whole region of Western Europe.

No theory of terrestrial temperature, then, can meet the requirements of observation which does not account for isothermal surfaces approximately parallel (with local variations) to the earth's external surface at comparatively small depths beneath it. Moreover, it is easily shown that the quantity of heat transmitted from such a surface to the external surface, must be proportional to the conductive power

of the superincumbent mass through which the transmission takes place (in the previous case the quantity of transmitted heat is independent of that power). Consequently, whatever may be the cause supplying the heat at depths not much exceeding the general aggregate depth of the sedimentary beds, it must furnish a *quantity of heat* proportional to the vertical flow of heat, *i. e.* a quantity proportional to the conductive power of the superincumbent mass. Thus the energy of the producing cause must have distinct relations to *superficial conditions.* Must not, then, the cause itself be at least partly *superficial,* and not entirely *central?* The author is convinced that such must be the case. He does not profess, in this paper, to carry his speculations further.

It should be remarked that the argument derived from the above investigations is not directly against the theory of a *primitive* heat, but only against the manifestation of the remains of such heat as the sole cause of existing terrestrial temperatures in the superficial crust of the globe, at depths beyond the sensible effect of the direct solar heat. Whatever may be the weight of the argument in favour of the earth's original fluidity (and therefore of its primitive heat), founded on the oblateness of its form, for example, the cogency of such argument remains unaltered. At the same time, all the collateral arguments in favour of primitive heat, founded on the existing temperature of the earth's crust, or the climatal changes which are believed to have taken place on its surface, are deprived, the author conceives, of nearly all their weight. Moreover, admitting only a part of the existing terrestrial heat to be due to superficial causes, the flow of heat from the earth's central portions must be less by that amount than if the whole flow were due to central heat. Consequently the rate of increase of terrestrial temperature *due to the flow of central heat* must be proportionally diminished, and the depth at which we should arrive at the temperature of fusion proportionally increased. The conclusion, therefore, that the earth's solid crust is as thin as some geologists have supposed it to be, as well as all theories based on that conclusion—whether of volcanic action, or of elevation and depression of the earth's surface—must be deprived of nearly all their force.

The remainder of this paper contains details of experiments, and descriptions of the apparatus used in making them.